LETTERS TO THE EDITOR.

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Sir Edward Fry on Natural Selection.

I ASK leave to make a few observations on Mr. Galton's letter under the above heading which appeared in your issue of February 12.

In my papers on the age of the inhabited world and the pace of organic change in the *Monthly Magazine* for last December and January, I had a passage on the difficulty which appeared to me to exist in conceiving mimetism to have been produced by the gradual accumulation of minute points of likeness. On this Mr. Galton observes that "two objects that are somewhat alike will be occasionally mistaken for one another when the conditions under which they are viewed are unfavourable to distinction." If by "somewhat alike" Mr. Galton means have some point of likeness, however minute, then the proposition would refute my objection; but it would, as I think, be manifestly untrue. If, on the other hand, by "somewhat alike" be meant a considerable likeness, then the proposition is manifestly true, but leaves unanswered the difficulty on which I have dwelt, viz. the difficulty of seeing how natural selection could have helped the organism to convert minute points of likeness in the midst of unlikeness into such a preponderance of likeness as to produce deception.

Mr. Galton has illustrated his point by the fact that "i" may often be mistaken by the beholder for "l," "k," or "h." But here he starts with an obvious and considerable likeness, and the question is, how could that degree of likeness be reached by natural selection?

Let us take two sheets of paper, the one a *tabula rasa,* the other covered with a thousand dots arranged so as to produce a highly complicated pattern. Then let dots appear successively, but sporadically, on the white paper in places where there are dots on the other paper, until, in the end, the two papers are indistinguishable. It seems to me to be obvious that for a long while no eye would mistake the one paper for the other; but that, as the process goes forward, a point will be reached where an occasional mistake will occur under conditions unfavourable to distinction. Now I agree that it is conceivable that from this point forward natural selection may operate, but as to the whole interspace between the first minute change that deceives no one to the point of first deception, it appears to me plain that natural selection cannot operate at all, and that the theory of the accumulation of minute variations, therefore, fails to account for the facts of mimetism in insects and other organisms.

If the two suggestions of sudden and great variation on the one hand, and of the slow accumulation of small variations on the other be considered as the possible explanation of the facts of mimetism, I cannot but think that the latter will be found far more probable than the former; and therefore, whilst willingly admitting the great weight to be attributed to the opinion of Mr. Galton on the subject, I remain unconvinced.

But suppose that on this point I am wrong and Mr. Galton is right, does he not judge my argument with undue severity when he treats it as "so faulty as to seriously compromise the value of the memoir as a whole"? My observations on mimetism are not the basis of my argument, which is a collection of facts which appear to show the existence of sudden and heritable variations. They are a part, and a separate part only, of an argument that the accumulation of minute variations will not account for some known facts attributed to it. The inculpated paragraph may be struck out of my paper, and all the rest will stand unaffected. Even if this error, if error it be, has compromised not a single passage only but the whole of my paper, I am glad to find that Mr. Galton is in sympathy with its general purport, and I thank him for the courteous language which accompanies his condemnation of my lapse.　　EDW. FRY.

Failand, February 23.

The Assumed Radio-activity of Ordinary Materials.

WITH reference to Mr. Strutt's recent article and Prof. J. J. Thomson's letter on this subject, may I venture modestly to urge that it may be well to consider whether the condition set up in air to which attention is directed be not the outcome of the occurrence of a minute amount of chemical change of an ordinary character—whether it be not a sort of Russell effect on an infinitely minute scale, detected by an infinitely delicate test? That oxidative change is in continual progress, I imagine, is the belief of everyone who has paid the slightest attention to the subject; and that leaf surfaces—if not waterfalls—are the certain seat of such changes may be regarded as unquestionable. Those of us who require something more than an attitude of papal infallibility in proof of a scientific proposition would like to see the old love honourably retired before the new one is accepted in society.　　HENRY E. ARMSTRONG.

The Dissociation Theory of Electrolysis.

IN a recently published "Text-book of Electrochemistry," by Svante Arrhenius, and translated by Dr. McCrae, I find on p. 114 of the translation the following statements:—

"Even when working with polarisable electrodes . . . the smallest fall of potential is sufficient to cause a current in the liquid. This fact was proved by Buff with currents so small that it was only after months that a cubic centimetre of explosive mixture was obtained. According to this the very smallest force is sufficient to split the molecules of the Grotthus chain . . . Faraday's view is therefore incorrect. The radicles of the salt molecule cannot be held together by a force of finite value."

The ideas of current and electromotive force are here rather mixed, but obviously the passage refers to a very necessary part of the foundation of the dissociation theory of electrolysis, viz. that a minute E.M.F. can evolve in the free state the ions of an electrolyte the heat of combination of which is considerable.

On referring to Buff's papers (*Lieb. Ann.,* lxxxv. p. 1 and xciv. p. 1), I find no mention of an evolution in the manner described of any explosive mixture whatever; taking this to mean free oxygen and free hydrogen evolved simultaneously by an E.M.F. less than that of one Daniell's cell. Indeed, towards the end of his second paper, Buff incidentally states that a single cell produces merely a polarisation which almost stops the current.

Surely in the attempt to found a theory in opposition to that of Faraday some modicum of care should be taken to verify the sources of information.

In "Outlines of Electrochemistry," by Prof. Harry C. Jones (1901), we find at p. 15 the same kind of statement, that the dissociation theory accounts for, and is founded on, experimental evidence, showing that "a very weak current" can decompose water and set free its constituents simultaneously. Here also the word "current" is used, though "E.M.F." is apparently meant.

No reference is given, but the statement occurs in a discussion of the well-known Clausius theory. In his description of this theory (*Pogg. Ann.,* ci. p. 338), Clausius certainly does not mention, and apparently did not believe, that any such phenomenon could occur.

It would be interesting to know if anyone has ever observed it.

At all events, the acceptance of the theory in question is certainly not encouraged by an encounter with such serious errors in the description of experiments put forward as its foundations.　　J. BROWN.

Analysis of the "Red Rain" of February 22.

SOME of your readers will probably be interested to know something of the nature of the muddy rain which fell here on Sunday, February 22. A sample of the downfall, caught in an open field between 10 a.m. and 12 noon, was brought to me to examine, and particulars of the partial analysis of the suspended matter which the water contained are subjoined. The large percentage of organic matter seems to me to be the most remarkable point in the analysis, and I regret not having had time to make a separate investigation of this. A rapid examination of the physical proper-

ties of the sediment, or mud, which I made, seemed to indicate that the organic matter was condensed hydro-carbon gases, or condensed volcanic vapours (such as one might expect to be evolved unburnt in a very large volcanic outburst). The sediment seems to be terrestrial, as the large amount of organic matter, coupled with the small amount of iron found, prohibits the theory of a meteoric origin.

The rain water contains 37·0 grains of suspended matter, or mud, to the gallon.

The analysis of the suspended matter, dried at 100° C., is as follows :—

Organic matter (loss on ignition)	...			36·4	per cent.
Silica	...	...	...	...	45·6	,,
Alumina and oxide of iron...			...	13·6	,,
Magnesia	...	...	...	...	2·4	,,
Unclassified	...	...	...	...	2·0	,,
					100·0	,,

Buckfastleigh, March 2.　　　　　　ROWLAND A. EARP.

Proof of Lagrange's Equations of Motion, &c.

IN your issue of January 29, Mr. Heaviside put forward a demonstration of Lagrange's equations of motion which appears invalid. As neither his interpretation of Newton nor his argument based thereon was stated with sufficient clearness to enable a critic to locate the weak spot without running serious risk of misinterpreting him, it seemed better in the first instance to point out a well-known case in which precisely similar reasoning would lead to Lagrange's equations of motion where they are known to be untrue (the reason, and a proper remedy, being also generally known). This I did in your number of February 19 ; his reply, in the same number, is to the effect that he does not intend to uphold the truth of Lagrange's equations in such a case. It is not, however, logically permissible for anyone to escape the inconvenient consequences of his own argument in such a fashion.

Possibly Mr. Heaviside has not grasped my point. If the argument he puts forward on p. 298 is valid, I am unable to see any point at which the following can without inconsistency be alleged to fail :—" In the case of a rigid body rotating round a fixed point with angular velocities ω_1, ω_2, ω_3 about its principal axes the kinetic energy T is a homogeneous quadratic function of the ω's, with coefficients which are constants. This makes

$$2T = \omega_1 \frac{aT}{d\omega_1} + \omega_2 \frac{dT}{d\omega_2} + \omega_3 \frac{aT}{d\omega_3} \qquad (8)$$

therefore

$$2\dot{T} = \omega_1 \frac{d}{dt}\left(\frac{dT}{d\omega_1}\right) + \dot{\omega}_1 \frac{aT}{d\omega_1} + \ldots \qquad (9)$$

But also by the structure of T,

$$\dot{T} = \dot{\omega}_1 \frac{dT}{d\omega_1} + \dot{\omega}_2 \frac{dT}{d\omega_2} + \dot{\omega}_3 \frac{dT}{d\omega_3} \qquad (10)$$

So, by subtraction of (10) from (9)

$$\dot{T} = \omega_1 \frac{d}{dt}\left(\frac{dT}{d\omega_1}\right) + \omega_2 \frac{d}{dt}\left(\frac{dT}{d\omega_2}\right) + \omega_3 \frac{d}{dt}\left(\frac{dT}{d\omega_3}\right) \qquad (11)$$

and therefore, by Newton, the torque about the first axis is the coefficient of ω, *i.e.* $A\dot{\omega}_1$, and similarly for the rest."

There is no step in his demonstration which requires that the coordinates should be " proper Lagrangian coordinates within the meaning of the Act "; in the proof usually given there is such a step.

It is with great diffidence, lest I may do Mr. Heaviside injustice through misinterpreting him, that I now venture to express the conjecture that in his argument he may possibly have failed, as is sometimes done [by Maxwell, for instance, " Treatise," second edition, § 561, equations (5)], to distinguish between the displacements which a 'material system actually receives during its motion and displacements which are perfectly arbitrary subject only to the geometrical connections of the system, and have thus confounded the equation

$$X_1 v_1 + \ldots = \left(\frac{d}{dt} \cdot \frac{dT}{dv_1} - \frac{dT}{dx_1}\right) v_1 + \ldots$$

which expresses that the rate at which work is done by the forces is equal to the rate at which the system gains kinetic energy, with the very different one

$$X_1 \delta x_1 + \ldots = \left(\frac{d}{dt} \cdot \frac{aT}{dv_1} - \frac{dT}{dx_1}\right) \delta x_1 + \ldots$$

in which δx_1, &c., are arbitrary displacements as above. When the latter equation is established, Lagrange's equations follow at once, but Mr. Heaviside has made out no case for deducing them from the former. In every case, as in the example I cited, the right-hand member of the former equation can be written in the form

$$v_1 \varphi_1(x_1, v_1, \dot{v}_1, x_2, v_2, \dot{v}_2, \ldots) + \ldots$$

in an infinite variety of ways, and accordingly it is sufficiently obvious that there is no warrant for stating that the force on x_1 is the coefficient of v_1 in any one such form more than in any other. Samples of expressions which might thus be wrongly obtained for the torque about the first axis in the instance alluded to are

$$A\dot{\omega}_1, \quad A\dot{\omega}_1 - (B-C)\omega_2\omega_3,$$
$$A\dot{\omega}_1 + (B-C)\omega_2\omega_3, \quad A\dot{\omega}_1 - (B\omega_2{}^3 - C\omega_3{}^3)/\omega_1.$$

For the simpler case of a particle moving in a plane, one could thus obtain, for example, the equations,

$$X = m(\ddot{x} - k\dot{y}), \quad Y = m(\ddot{y} + k\dot{x}),$$

where k is any quantity whatever.

In short, the latter of the two equations compared above differs from the former in being equivalent to a set of independent equations equal in number to that of the coordinates of the system.

Similar remarks apply, of course, to his treatment of the question of an elastic medium, p. 297.

That the Principal of Energy, or of Activity, does not by itself afford a sufficient basis from which to formulate the fundamental equations of dynamics in any form whatever is admitted almost universally ; from Mr. Heaviside's letters it appears at least doubtful whether he is willing to agree with this general and well grounded opinion ; he has advanced no valid argument against it, however.　　　　　　　W. McF. ORR.

February 22.

A FEW weeks ago you published in a letter from Mr. Heaviside a proof of Lagrange's equations of motion of a system of bodies. I must confess that I in common with others swallowed it, but I have now come to the conclusion that the proof, though doubtless admirable as an example of the power of the " Principle of Activity," does not prove Lagrange's equations. In fact, if q be a coordinate, $\dot{q}$ the corresponding velocity, and Q the corresponding force, we have the result

$$\Sigma \dot{q} \left\{ \frac{d}{dt} \frac{\partial T}{\partial \dot{q}} - \frac{\partial T}{\partial q} - Q \right\} = 0$$

for any possible motion of the system. But we are not entitled to equate the quantities in the brackets to zero, for *these are not independent of $\dot{q}$.* The " proof " is, in fact, merely Maxwell's well-known but fallacious proof, simplified by going direct instead of *viâ* Hamilton.

Cambridge, February 28.　　　　　　　R. F. W.

Genius and the Struggle for Existence.

PERMIT me to point out that Dr. A. R. Wallace's statement (p. 296), " the comparatively short lives of millionaires," is not supported by facts, at any rate by those for the last three years.

The following has been obtained from the details concerning estates on which death duties were paid. Nine millionaires died during 1900, leaving in the aggregate 19 millions. The average age of these nine testators is seventy-four—the youngest was fifty-nine and the oldest ninety-one years.

During 1901, we find that the deaths of eight millionaires are recorded, whose joint estates were valued at $10\frac{1}{2}$ millions. In this case too, we find that the average age is above the allotted threescore years and ten, being seventy-two. The